Every one must admire, and praise the skill which fashioned, the vacuum receivers for storing and experimenting with comparatively large quantities of liquid oxygen and air, which Prof. Dewar used in public for the first time on January 20, 1893, so far as can be gathered from the *Proceedings* of the Royal Institution (see *Proc. R. I.* xiv. 1.) The following sentences occur in the published abstract of Prof. Dewar's lecture on that date: "The prosecution of research at temperatures approaching the zero of absolute temperature is attended with difficulties and dangers of no ordinary kind. *Having no recorded experience to guide us in conducting such investigations*, the best instruments and methods of working have to be discovered." (The italics are mine.) (*Proc. R. I.* xiv. 1.) Now, in June 1890, that is two and a half years before Prof. Dewar made the statement I have quoted, Prof. Olszewski was able to say: "My new apparatus excludes the inconveniences of the former one, and renders it possible to preserve the liquid oxygen a longer time under the ordinary atmospheric pressure." And again: "Thus was solved the problem of liquefying considerable quantities of oxygen without the slightest danger." (See Olszewski, *Phil. Mag.* February 1895, 193-3.) Every reader of Prof. Olszewski's paper will agree that these statements are justified. Moreover, in October 1891, that is, fifteen months before Prof. Dewar declared there was "no recorded experience to guide us in conducting such investigations," Prof. Olszewski (in conjunction with M. Witkowski) described a method for not only storing, but also experimenting with, considerable quantities of liquid oxygen (*Cracovie Bulletin*, October 1891). So far back as 1887 he published, in *Wiedemann's Annalen* (xxxi. 58), a full account of methods for determining the densities of liquid oxygen, nitrogen, and marsh gas. In 1887, also, he determined the boiling point of liquid ozone, and he began his measurements of the absorption spectra of liquid oxygen and air (*Wiedemann's Annalen*, xxxiii. 570); and in 1891 he published further measurements of the absorption spectrum, and the refractive index, of liquid oxygen (*ibid.* xlii. 663; see also *Phil. Mag.* February 1895, 197-9, and 206-8.)

It is just these properties, namely, the optical properties, of liquid oxygen which had been elaborately studied by Prof. Olszewski from 1887 to 1891 that are largely dwelt on by Prof. Dewar in his discourse of January 1893; and it is the illustration of these properties that is prefaced by the remark, there is "no recorded experience to guide us in conducting such investigations."

On January 20, 1893 (*Proc. R. I.* xiv. 10) Prof. Dewar said that liquid oxygen is "the most convenient substance to use for the production of temperatures about – 200° C. Liquid nitrogen, carbonic oxide, or air can conveniently be made at the ordinary atmospheric pressure, provided they are brought into a vessel cooled by liquid oxygen boiling under the pressure of about half an inch of mercury." It is interesting to compare with this Prof. Olszewski's words published in June 1890 (*Crac. Bull.*; I quote from the translation in *Phil. Mag.* February 1895, 192): "I proved long ago [*Compt. rend.* c. 350 (1885)] that liquid oxygen is the best cooling agent; for it easily gives the temperature of – 211°·5 C. if the pressure is lowered 9 mm. of mercury, and it does not freeze even at the pressure of 4 mm." It may be said that the similarity between these statements is merely a coincidence. Perhaps it is. Nevertheless, Prof. Dewar's statement conveys the impression that he was the first experimenter to employ liquid oxygen for obtaining, and maintaining, very low temperatures. Another of Prof. Dewar's statements which imply more than they express, occurs in his lecture of June 10, 1892 (*Proc. R. I.*, xiii. 695): "He hoped that evening to go several steps further, and to show liquid air, and to render visible some of its more extraordinary properties." Remembering that air had been liquefied by Prof. Olszewski at least eight years before the lecture wherein this statement occurs was delivered, one must regret that Prof. Dewar did not choose words which should have made it impossible for the English public to suppose "that the liquefaction of oxygen and other so-called permanent gases was achieved for the first time by [him]." (See Prof. Olszewski's letter in NATURE of January 10).

Not only does Prof. Olszewski claim to have devised the methods, and constructed the apparatus, for liquefying considerable quantities of the more permanent gases, but he also asserts that most of the experiments on the properties of these liquefied gases which have been performed by Prof. Dewar are only repetitions, on a larger scale, of work done by others. What, then, are these experiments?

Measurements of the refractive index of liquid oxygen, for the sodium light, and also of the absorption spectrum of liquid oxygen, were published by Profs. Liveing and Dewar in August 1892 (*Phil. Mag.* (5) xxxiv. 205; see also Dewar in *Proc. R. I.*, June 10, 1892; and Liveing and Dewar in *Phil. Mag.* for August 1894). The refractive index of liquid oxygen had been measured, and determinations of the absorption spectrum had been made, by Olszewski and Witkowski in October 1891, and these measurements were the complement of work begun, and published, by Prof. Olszewski in 1887 (*Wiedemann's Annalen*, xxxiii. 570. See Olszewski in *Phil. Mag.*, February 1895, 197-9). It is true that preliminary experiments on the absorption spectrum of liquid oxygen had been made by Profs. Liveing and Dewar in 1889. In their paper in *Proc. R. S.*, June 6, 1889, they say:—"We have observed repeatedly the absorption of liquid oxygen in thicknesses of 8 and 12 mm. Our observations confirm those of Olszewski."

On December 10, 1891, Prof. Dewar announced, in a note to the President of the Royal Society, that liquid oxygen is attracted by the poles of a magnet. This note was followed by another, on December 17, 1891, saying that liquid ozone was also attracted by the poles of a magnet. These detached, but interesting, experiments, which followed up work done by Faraday many years ago, must be placed to the credit of the Fullerian Professor at the Royal Institution.

A paper published in *Phil. Mag.* (5) xxxiv. 326 (1892) by Dewar and Fleming, on the electrical resistances of metals and other bodies at very low temperatures, contains the only piece of exact investigation to which Prof. Dewar's name is attached wherein liquid oxygen or air is used as an instrument of research, with the exception of the measurements of the optical properties of liquid oxygen, which have been shown to be mainly repetitions of the work of Olszewski and Witkowski. This research carries on and supplements earlier work done by Cailletet and Bouty, and by Wroblewski (*Comptes rendus*, ci. 161 (1885).

A few observations have been made by Prof. Dewar on the phosphorescence of various substances at the temperature of boiling oxygen, and on the cessation of chemical action, and the continuance of photographic action, at the same temperature (see *Proc. R.I.* June 26, 1891 and June 10, 1892, also January 20, 1893; also *Proc. Royal Soc.* April 19, 1894, and *Proc. Chem. Soc.* June 28, 1894). The subject of chemical action at very low temperatures was investigated by Pictet in May and November 1892 (*Comptes rendus*, cxiv. 1245; cxv. 814) in a much more exhaustive way than has been done by Dewar, who has only touched the fringe of the matter (compare also Olszewski, *Phil. Mag.* February 1895, 208-9). The observations on the sensitiveness of the Eastman film at very low temperatures are interesting; but, so far as it has been published, the work is too slight to present material for detailed criticisms; and the same may justly be said of the lecture illustrations of phosphorescence at low temperatures. It may be remarked that Prof. Dewar's paper on photographic action at low temperatures has appeared only in the *Proceedings*, not in the *Transactions*, of the Chemical Society.

Prof. Olszewski's work on the properties of liquefied gases was begun, and accounts of his accurate experiments were published in recognised scientific journals, long before Prof. Dewar's experiments were heard of. The work of the Polish Professor is being continued in the same exact, modest, and scientific manner (see his paper on the optical dispersion of liquid oxygen in *Cracovie Bulletin*, July 1894, noticed in *Phil. Mag.*, February 1895, 208; and his determinations of the boiling and freezing points, and other constants, of argon communicated to the Royal Society, January 31, 1895).

It seems to me that Prof. Olszewski has established a case which demands the instant and serious consideration of those who are truly interested in the advance of science, and are jealous of the good name of the scientific men of this country.

Cambridge, February 7. M. M. PATTISON MUIR.

THE object of the communications on the liquefaction of gases, which have recently appeared in NATURE and the *Philosophical Magazine*, is to depreciate the work of Cailletet and Pictet, to smother away the first-class work of the deceased

Wroblewski to annihilate myself, and thereby to magnify the claims for originality of Prof. Olszewski. In spite of the mistakes inevitable to pioneers, the work of Cailletet and Pictet must always be credited with originating research in this department. To show my appreciation of such investigation, and to give it a wide publicity in the year 1878, a Friday evening address was devoted to the work of Cailletet and Pictet, during the course of which I showed for the first time in this country the working of the Cailletet apparatus. Similarly, in the year 1884, an address was given on the work of Wroblewski and Olszewski, in the course of which I illustrated for the first time in public the liquefaction of oxygen and air, showing the boiling point, &c., by means of a simple form of apparatus which did not involve the use of the Cailletet pump as employed by these experimenters. To deliver a lecture on the work of other people is generally considered a mark of honour, and as usefully diffusing a knowledge of the same. The critic has for some object omitted half the opening sentence of this address, which runs as follows :—

"The two Russian chemists, MM. Wroblewski and Olszewski, who have recently made such a splendid success in the production and maintenance of low temperature, have used in their researches an enlarged form of the well-known Cailletet apparatus ; but for the purpose of lecture demonstration, which necessarily involves the projection on a screen of the actions taking place, the apparatus represented in the annexed woodcut is more readily and quickly handled, and enables comparatively large quantities of liquid oxygen to be produced."

The same tactics have been adopted of taking half a sentence from another part of the address, and leaving out the context. The sentence to which I refer ran thus :—

"*Provided a supply of liquid ethylene can be had, there is no difficulty in repeating all the experiments of the Russian observers ;* but as this gas is troublesome to make in quantity, and cannot be bought like carbonic acid or nitrous oxide, such experiments necessitate a considerable sacrifice of time. It was therefore with considerable satisfaction that I observed the production of liquid oxygen by the use of solid carbonic acid, or, preferably, liquid nitrous oxide."

Then follows a full description of how to use nitrous oxide with the apparatus in order to get liquid oxygen. Note the disingenuous comment of the critic : "No claim is made here to originality in the essentials of the apparatus, nor in the experiments performed." It has never been the custom to make public claims for originality, either in apparatus or experiment, in the course of an address given at the Royal Institution. He proceeds to allege, "the apparatus referred to by Prof. Dewar in his lecture of June, 1884, was used by Prof. Olszewski in 1883." On turning to the reference given, I find the following statement by Olszewski : "In 1883, and for several years following, I liquefied the gases in a strong glass tube." According to this distortion of fact, the apparatus of Andrews, Cailletet, Wroblewski, and Dewar are all the same, because gas was liquefied in a glass tube. I challenge the production of a reference to an apparatus antecedent to the date of this lecture in which liquid nitrous oxide had been used for the production of liquid oxygen. But all this is ancient history ; the real charge begins with the year 1890. Assuming that I had been the thief of Prof. Olszewski's apparatus of 1890, which is alleged to be the type used in the Faraday Commemoration Lecture of 1891, *what explanation is forthcoming of the delay of four years in ventilating the question of priority and personal grievance? What has occurred at the present juncture to precipitate a crisis?* Can any one avoid coming to the conclusion that Prof. Olszewski's contact with "Argon" explains the sudden assault upon my labours in the same field of investigation. . . .

If the critic had been a trustworthy person even to examine into the published facts, he could have found a reference in the *Proceedings* of the Royal Institution, between the years 1878 and 1893, that would have enlightened his ignorance, and thereby prevented his reiterating Prof. Olszewski's suggestion that my large apparatus of 1891, which produced pints of liquid oxygen, was copied or borrowed from a description in the *Cracovie Bulletin* for 1890. . . .

Let us see what Prof. Olszewski said in that paper, entitled "Transvasement de l'oxygene liquide," June 1890 :—

"A flask of wrought iron, five litres in capacity (such as is used to hold liquid carbon dioxide), containing oxygen under a pressure of 80 atm., is joined by a narrow copper tube to the upper end of a steel cylinder tested at a pressure of 200 atm. This cylinder having a capacity of 30-100 cub. centim., according to the quantity of oxygen which we wish to liquefy at a time, is immersed in liquid ethylene, of which the temperature may easily be lowered to 140 c. by means of an air-pump. The lower end of the cylinder is joined by a narrow copper tube to a little stopcock, through which the oxygen, liquefied in the cylinder, can be poured down into an open glass vessel, kept cool by the surrounding air."

In other words, replace the glass tube in my apparatus of 1884 by a small steel cylinder, and attach to its lower end a narrow copper tube with a stopcock, and the Olszewski apparatus of 1890 is produced (as a matter of fact, Pictet had used the same principle in the year 1878). Now, on June 11, 1886, I delivered a lecture on "Recent Researches on Meteorites," and the report in the *Proceedings* of the Royal Institution contains a sectional drawing of an apparatus solely constructed of copper, together with a valve for drawing off liquid oxygen, entirely different in type from the crude plan Olszewski adopted in 1890. I may mention that the plan of the apparatus was reproduced immediately after the delivery of the lecture, both in England and America. The section is confined to the refrigerator, all the accessories of liquefied and compressed gas bottles, compression and exhaust gauges, &c., having been omitted. From this plan of the refrigerator, any person so desiring could increase its capacity so as to work on a larger scale. The drawing shows the apparatus arranged for the special experiment of ejecting liquid oxygen into a vacuum chamber, but it is clear the apparatus discharged as easily into an open vessel. It is not any isolated experiment that is in dispute, but the new form of apparatus. The special object for which liquid oxygen was in use in this lecture, was to cool a piece of meteorite before insertion into an electric furnace. The following extract from the lecture will explain how the subject was introduced :—

"Meteorites, no doubt, have an exceedingly low temperature before they enter the earth's atmosphere, and the question had been raised as to what chemical reactions could take place under such conditions. It resulted from Prof. Dewar's investigations that at a temperature of about 130° C., liquid oxygen had no chemical action upon hydrogen, potassium, sodium, phosphorus, hydriodic acid or sulphydric acid. It would appear, therefore, that as the absolute zero is approached, even the strongest chemical affinities are inactive.

"The lecturer exhibited at work the apparatus by which he had recently succeeded in solidifying oxygen. The apparatus is illustrated in the accompanying diagram, where a copper tube is seen passing through a vessel kept constantly full of ether and solid carbonic acid ; ethylene is sent through this tube, and is liquefied by the intense cold ; it is then conveyed by the tube, through an india-rubber stopper, into the lower vessel ; the outer one is filled with ether and solid carbonic acid. A continuous copper tube, about 45 feet long, conveying oxygen, passes through the outer vessel, and then through that containing the liquid ethylene ; the latter evaporates through the space between the two vessels, and thus intense cold is produced, whereby oxygen is liquefied in the tube to the extent occasionally of 22 cubic centimetres at one time. The temperature at which this is effected is about -130° C., at a pressure of 75 atmospheres ; but less pressure will suffice. When the oxygen is known to be liquid, by means of a gauge near the oxygen inlet, the valve A is opened, and the liquid oxygen rushes into a vacuum in the central glass tube below ; some liquid ethylene at the bottom of the next tube outwards is also caused to evaporate into a vacuum at the same moment, and instantly some of the liquid oxygen in the central tube becomes solid, owing to the intense cold of the double evaporation. The outer glass vessel serves to keep moisture from settling on the sides of the ethylene tube. By means of the electric lantern and a lens, an image of this part of the apparatus was projected upon the screen, this being the first time that the experiment had been shown on a large scale in public.

"Performing this experiment, the temperature reached was a little below 200° C., that is, only 50° or 70° above the absolute zero of temperature, and in the experiment about 5 lbs. of liquid ethylene were employed."

This I declare to be the first apparatus made wholly of metal, being an arrangement of copper coils, in which liquid oxygen was made and decanted or transferred from the vessel in which it was liquefied to another by means of a valve, and thereby rendered capable of use as a cooling agent. In support of this assertion, I call as witness Prof. Charles Olszewski himself, who states in the *Philosophical Magazine*, February 1895, p. 189: "In 1883, and for several years following, I liquefied the gases in a strong glass tube." There is no suggestion made that a steel cylinder and valve was used by Olszewski till the year 1890. Whereas four years in advance I had used a much safer and better form of apparatus, practically identical in principle with that used in Cracow in the year 1890. Have I ever suggested that Prof. Olszewski was anticipated, or attempted to raise any question of priority? Perhaps the critic will have the audacity to say, in reply, this is no publication, the *Proceedings* of the Royal Institution, English and American science periodicals, not being amongst the class of recognised scientific journals. Well, if I am pleased to throw my bread upon the waters, adopting the view that every truthfully recorded experiment which appears in any journal associated with my name is publication, surely I should simply be conducting myself in the "too modest" way my critic commends.

As a specimen of the distortion of facts to prove another case of priority that is claimed, I find that MM. Charles Olszewski and Auguste Witkowski, Membres Correspondants, presenting their memoir "Propriétés optique de l'oxygene liquide," on October 3, 1892, and, on referring to the paper, it is dated July 15, 1892, and the following footnote is added:

"Avant la publication de notre communication, MM. Liveing et Dewar ont fait connaitre (*Phil. Mag.* Aout 1892), les resultats de leurs recherches sur la refraction des gaz liquéfiés."

Yet the critic says our experiments were "mainly repetitions of the work of Olszewski and Witkowski." The garbled extracts selected to make it appear that I have been guilty of misrepresentation are all of the same kind. . . . Thus I am taken to task for using the expression in the lecture on liquid air, of 1893: "Having no recorded experience to guide us in conducting such investigations, the best instruments and methods of working have to be discovered." The next sentence runs as follows: "The necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination. Apart altogether from the rapid ebullition interfering with the experimental work, the fact that it did take place involved a great additional cost in the conduct of experiments on the properties of matter under such exceptional conditions of temperature." What can be said in defence of such glaring misrepresentation of the meaning of my words? Mr. M. M. Pattison Muir's demand for "*instant and serious consideration*" of his client's "*case*" has been quickly met. I trust the result will . . . . fit in with his brief.

JAMES DEWAR.

Royal Institution, February 12.

[A few personal remarks in Prof. Dewar's letter have been omitted, as they do not affect the points at issue.—ED. NATURE.]

Vertebrate Segmentation.

MR. H. G. WELLS, in a recent number of NATURE, honours my little book by making it an example of a contravention of what he regards as a principle of education. With that I have no quarrel. But I must object to the instance he has chosen. The sentences from which he quotes refer to the phenomena of segmentation common to cœlomate tissues, and not to the derivation of vertebrates from any invertebrate group. So far from giving "the impression almost in so many words—'cut and dried,' and ready to be cast into the oven—that the vertebrate type is merely a concentrated derivative (concertina fashion) of the chætopod type," I devote the chapter (xv.) from which he has taken his quotations, to showing that the earthworm and the vertebrates merely belong to two out of the many isolated groups; and at the end of the chapter (though not in spaced type, as I did not consider the question of vertebrate descent congruous with the aims of an elementary text-book) I state that "the type common to the lowest members of the groups of whtch the earthworm on the one hand, and the vertebrates on the other, form the highest examples, is a simple unsegmented cœlomate animal."

P. CHALMERS MITCHELL.

The Black-veined White Butterfly.

MR. KIRBY, on p. 340 of your last issue, says (in criticism of Mr. Furneaux) that this insect "would not frequent open ground at a distance from trees." I suppose there are not now many Englishmen who have taken it in this country; and it may be worth while to record that the common on which my brother and I used to find it tolerably abundant in the years 1857–1859, was quite an open place, with no adjacent wood, and very little hedge timber. This common is about a mile and a half to the west of Cardiff; I passed it in the train a few weeks ago, and noted that it is being encroached on by suburbs. We had many a hot chase there over gorse and briar, and always considered this butterfly the most difficult of all to catch. I have never seen it in England since 1859 or 1860.

Oxford, February 11.

W. WARDE FOWLER.

Parrots in the Philippine Islands,

PRAY allow me space to acknowledge a bad mistake which I first made in the ninth edition of the "Encyclopædia Britannica" (xviii. p. 322), and have lately repeated in the "Dictionary of Birds" (p. 687), by asserting that parrots are "wanting in the Philippine Islands." Seeing that the article was written more than ten years ago, it is quite out of my power to account for the misstatement: my only wonder is that it has not been before challenged, since there is, and has been for some centuries, abundance of evidence to show that there are plenty of parrots in that group of islands, which, indeed, is as well furnished with them (as remarked by my friend Mr. L. W. Wiglesworth, who has kindly drawn my attention to my error) as is the island of Celebes, and I had already (p. 93) noticed the Philippine species of Cockatoo.

Cambridge, February 9.

ALFRED NEWTON.

TWENTY FIVE YEARS OF GEOLOGICAL PROGRESS IN BRITAIN.

LOOKING back across the fourth part of a century in the progress of any branch of science, we naturally turn first to the list of names of those to whose labours that progress has been due, and though many of these names may happily still be counted among the living, we note many a blank where the hand of death has thinned the ranks. Perhaps in this country no department of natural knowledge can boast a more illustrious bead-roll than that of Geology. The story of the earth had hardly begun to be scientifically studied until the first decades of the present century, and some of the early fathers of geology lived on until well within the life-time of the present generation. A curious transition has thus been going on during the last five-and-twenty years. On the one hand, there have been moving amongst us geological magnates who achieved their fame in the old days when it was still possible for a man to possess a tolerably full personal knowledge of almost every department of the science. On the other hand, around these few living memorials of the heroic age, grew up hosts of younger men, who, finding the main lines already traced for them, have become in large measure specialists, devoting themselves with enthusiasm, but with more restricted vision, to one formation, or one group of rocks, or one tribe of fossils. The days of broad outlines and rapid generalisation have gone. No new systems remain to be added to the geological record of these islands. No new assemblages of extinct types of life now reward the sedulous collector. We have entered upon the era of minute detail and patient elaboration. The field-glass has given way to the microscope. The advance of the science must now be based on laborious research, less brilliant no doubt in its immediate effect, but probably not less lasting in its influence and its results.